Is Education Inherited?
Understanding Intergenerational Transmission of Human Capital

by

Sandra E. Black
Department of Economics
UCLA, IZA and NBER

Paul J. Devereux
Department of Economics
UCLA

Kjell G. Salvanes
Norwegian School of Economics and Business Administration and IZA

August 2003

Preliminary and Incomplete

*Author contact information: Black: sblack@econ.ucla.edu; Devereux: devereux@econ.ucla.edu; Salvanes: kjell.salvanes@nhh.no. The authors would like to thank Marina Bassi for helpful research assistance.

Abstract

Parents with higher education levels have children with higher education levels. However, is this because parental education actually improves the outcomes of children, suggesting an important spillover of education policies, or is it merely that more able individuals also have more able children? This paper proposes to answer this question by using a unique dataset from Norway. Using the reform of the education system in the 1960s as an instrument for parental education, we find little evidence of a causal relationship between parents' education and children's education, despite significant OLS relationships. These findings suggest that high correlations between parents' and children's education are due primarily to selection and not causation.

I. Introduction

Parents with higher education levels have children with higher education levels. But why? There are a number of possible explanations. One is a pure selection story; the type of parent who gets more education, earns higher salaries, etc, has the type of child who will do so as well, regardless. Another story is one of causation; having more education makes you a different type of parent, perhaps better. Which means your children will have better outcomes as well.

Distinguishing between these two stories is important from a policy perspective. One of the key roles of publicly provided education in our society is to increase equality of opportunity. Many policy changes have taken place to further that goal in recent years.1 A key argument for proponents of this type of equality is the intergenerational transmission of education and opportunity; having more educated citizens will have longer run effects by improving the outcomes of their children. While many argue that this is the case, there is little causal evidence to suggest it is true.

The research to date has been very limited in its ability to distinguish between selection and causation. This paper proposes to solve this puzzle by using a unique dataset from Norway. During the period from 1960 to 1990, the Norwegian education system went through several large reforms. The primary reform occurred in the 1960s, when there was a drastic change in the compulsory schooling laws affecting primary and junior secondary schools.[2] Pre-reform, the Norwegian education system required children to attend school from the age of seven to the age of fourteen. After the reform, this was extended to the age of sixteen, adding two years of required schooling. Additionally, the

1 For example, in the United Kingdom, the Education Maintenance Allowance provides poor teenagers with a weekly allowance if they stay in school after the compulsory schooling age.

reform occurred over an extended period of time, starting in 1960 and continuing through 1971, where different municipalities in Norway adopted the reform at different times, allowing for regional as well as time variation. Evidence in the literature suggests that these reforms had a large and significant impact on educational attainment which, in turn, led to a significant increase in earnings (See Aakvik, Salavanes, and Vaage, 2003).[3] As a result, one can use this reform as a source of variation in parental education exogenous to parental ability in order to determine the impact of increasing parental education on children's schooling decisions. Although the instrument will only enable us to determine the impact on children's education of increasing parental education from seven to nine years, this may be an important starting point for identifying this intergenerational transmission of education.

Using this reform as an instrument for parental education, we find little evidence of a causal relationship between parents' education and children's education, despite significant and large OLS relationships. We find a small but significant causal relationship between mother's education and son's education but no causal relationship between mother's and daughter's educations or father's and son's or daughter's. This suggests that high correlations between parental and children's education are due primarily to selection and not causation.

The paper unfolds as follows: Sections 2 and 3 will discuss relevant literature and describe the Norwegian Education reform. Section 4 describes our empirical strategy; while sections 5 and 6 discuss the data we use and present our results. Section 7 discusses the various robustness and specification checks. Section 8 concludes.

---

[2] See Aakvik, Salvanes, and Vaage (2003) for more detail about the Norwegian educational reform.

3 Results on the impact on educational attendance of similar reforms also exists for Sweden, see Meghir

II. Background Information

There is an extensive literature on intergenerational transmission of income in the United States focusing on the correlation of parental and children's permanent income.[4] In work comparing Scandinavian countries with the United States, the evidence suggests that the more compressed is the income distribution, the smaller the correlation between parental and child outcomes. Bjorklund and Jantti (1997) compare intergenerational mobility in the U.S. and Sweden and find some evidence that intergenerational mobility is higher in Sweden. Bjorklund et al. (2002) extend this to include a comparison of intergenerational mobility in the U.S. to Norway, Denmark, Sweden and Finland and find support for higher income mobility in all the Scandinavian countries than in the US. Bratberg, Nilsen, and Vaage (2002) also explore the relationship between income inequality and mobility, focusing specifically on Norway, and argue that the compression of the earnings structure in Norway has increased intergenerational earnings mobility. F

Few studies analyze intergenerational mobility in education. Results from the US and UK suggest intergenerational education elasticities between 0.20 and 0.45 (Dearden et al., 1997; Mulligan, 1999). Using brother correlations in adult educational attainment as an overall measure of family and neighbourhood effects, Raaum, Salvanes and Sorensen (2001) find results for Norway that are similar to those Solon (1999) reports for the US. None of these studies, however, attempts to distinguish a causal relationship.

In recent work, there has been some effort to distinguish causation from mere correlation in ability across generations. Three broad approaches have been used:

---

and Palme (2003) and for England and Ireland, see Harmon and Walker (1995) and Oreopulos (2003).

[4] See review article by Solon (1999).

Identical twins, adoptees, and instrumental variables. Behrman and Rosenzweig (2002) use data on pairs of identical twin parents to "difference out" any correlation attributable to genetics. Simple OLS estimates, even controlling for father's schooling and father's log earnings, suggest a positive and significant relationship between mother's and children's schooling (with a coefficient of 0.13 on mother's schooling and coefficient of 0.25 on father's schooling, both significant). However, once one looks within female monozygomatic twin pairs, thereby differencing out any genetic factors that influence children's schooling, the coefficient on mother's schooling turns negative and almost significant. At the same time, the positive coefficient on father's schooling is reduced by over half to .11 and is no longer statistically significant. Recent work by Antonovics and Goldberger (2003), however, calls into question these results and suggests that the findings are quite sensitive to the coding of the data. Also, it may be unrealistic to assume that twins differ in terms of education but not in terms of any other characteristic or experience that may influence the education of their offspring (see Griliches, 1979 and Bound and Solon, 1999 for demonstrations that biases using twin fixed effects may be as big or bigger than OLS biases).

Plug (2002) uses data on adopted children to try to get at the causal relationship between parental education and child education. The idea is that if children are randomly placed with adoptive parents, the relationship between parental education and child education cannot simply reflect genetic factors. He finds a positive effect of father's education on child education but no significant effect for mothers. There are a few problems with this approach: Sample sizes are tiny, children are not randomly placed with adoptive parents, and the correlation between parents' education and child education

could be picking up the effects of any unobserved parental characteristic (patience, ability) that influences child outcomes.

Closest to our paper is a paper by Chevalier (2003), who uses a change in the compulsory schooling law in Britain in 1957 to identify the effect of parental education on their offspring. He finds a large positive effect of mother's education on her child's education but no effect of paternal education. This paper is limited in a few ways. There is no cross-sectional variation in the British compulsory schooling law. Chevalier uses cohorts of parents born between 1948 and 1967 and instruments parental education with a dummy for whether they were affected by the law change, year of birth, and the interaction of year of birth with the law dummy. Thus, he assumes away the presence of cohort effects in the second stage and the identifying variation in parental education arises both from secular trends in education and the once-off change in the law.[5] Second, the sample only includes children who are still living at home with their parents and hence loses some observations in a non-random fashion.6

III. The Norwegian Primary School Reform

In 1959, the Norwegian Parliament legislated a mandatory school reform. The reform was characterized by three broad goals, as stated explicitly in several government documents: 1) to increase the minimum level of education in society by extending the number of compulsory years of education from 7 to 9 years, 2) to smooth the transition to

5 This may be a particular problem in this context as less-educated individuals are more likely to have children while young and so in a sample of individuals with children of a certain age, older individuals are likely to have more education. Thus, one would like to control for unrestricted age effects for parents.
6 In related work, Currie and Moretti (forthcoming) find a positive effect of mothers' educational attainment on children's health outcomes in the U.S. using instrumental variables and fixed effects approaches.

higher education, and 3) to enhance equality of opportunities both along the socio-economic dimension and in particular the geographical dimension. Prior to the reform, children started school at the age of seven and finished compulsory education after seven years, i.e. at the age of fourteen. In the new system, the starting age is still seven years old, but the time spent in compulsory education is now nine years. The nine years are divided into two levels; first, six years of primary school, followed by three years of secondary school which prepared for high school.7 In contrast, prior to the reform, traditional education was divided into 7 years of mandatory schooling, 2 years of middle school, and 3 years of high school. After the 7 years of mandatory schooling, individuals could also opt out of traditional education preparing for university and choose different vocational training programs. In addition, the reform standardized the curriculum and increased access to schools since 9 years of mandatory school was eventually made available in all municipalities.

The parliament mandated that all municipalities (the lowest level of local administration) must have implemented the reform by 1973. As a result, although it was started in 1960, implementation was not completed until 1972.[8] Hence, for more than a decade, Norwegian schools were divided into two separate systems, and which system you were in depended on the year you were born and the municipality in which you lived. The first cohort that could have been involved in the reform was the one born in 1947. They started school in 1954, and (i) either finished the pre-reform compulsory school in

[7] The Norwegian school system has been slightly changed recently by the so-called "Reform97". Children now start at the age of six and the time spent in compulsory education is ten years, of which seven are at primary school and three are at secondary school. In addition, three years are available to all students either preparing for university or for a trade (vocational).

[8] The reform had already started on a small and explorative basis in the late 1950s, but applied to a negligible number of students because only three municipalities, each with a small number of schools, were

1961, or (ii) went to primary school from 1954 to 1960, followed by the post-reform secondary school from 1961 to 1963. The last cohort who could have gone through the old system was born in 1959. This cohort started school in 1965 and finished compulsory school in 1972.[9]

To receive funds from the state to implement the reform, municipalities had to present a plan for the new school in terms of buildings and funding to a committee under the Ministry of Education. Once approved, the costs of teachers and buildings would be provided by the state. While the criteria determining selection by the committee is somewhat unclear, the committee did want to ensure that implementation was representative of the country, conditional on having an acceptable plan. (Telhaug, 1969, Mediås, 2000). Figure 1 presents the spread of the reform over time, focusing on the number of municipalities implementing the reform per year.

While it is not necessary for our estimation strategy, it would be useful if implementation of the reform across municipalities were random over time with respect to the choice of parents' educational attainment. One might worry that poorer municipalities would be among the first to implement the reform, given the substantial state subsidies, while wealthier municipalities would move much slower. However, there has been work examining the determinants of the timing of implementation (See Lie 1973, 1974) that finds no relationship between municipality characteristics such as average earnings, taxable income, and educational levels, and the timing of implementation. Municipalities that are located geographically near municipalities that

---

involved. See Lie (1974), Telhaug (1969), and Lindbekk (1992), for descriptions of the reform.

[9] Similar school reforms were undertaken in most other European countries in the same period, notably Sweden, the United Kingdom and, to some extent, France and Germany (Leschinsky and Mayer, 1990).

already implemented the reform are themselves more likely to implement the reform; numerous interviews revealed that this is likely due to a particularly effective county administrator. As a result, the research supports a complex adoption process without finding support for single important factor to explain the implementation process. To examine this ourselves, Figures 2, 3, and 4 examine the implementation of the reform by the average income, parental education, and size of the municipalities; these figures suggest that there is little relationship between these factors and the timing of the implementation of the reform.

As a more rigorous test, in Appendix Table 1 we regress the year of implementation against different background variables based on municipality averages, such as parental income and the level of education, age, size of the municipality, etc. Consistent with the earlier literature, there appears to be no systematic pattern in the introduction rate in relation to parental average earnings, education levels, age, urban/rural status, and county dummies.[10]

IV. Identification Strategy

To identify the effects of parent education on child education, it is useful to have variation in parents' education that is exogenous to parental ability and other factors that are correlated with both parents' and child's educational choice. Our source of exogenous variation is an education reform in Norway that increased the number of years of

[10] There are 20 counties in Norway. Although an even development of the reform by city/rural, rich/poor areas was wanted by the reform committee, and we do not find any pattern, we cannot completely rule out the allocation being systematic in relation to relevant factors. For instance, systematic action on behalf of parents (migration to municipalities with the preferred education system, etc.) cannot be totally ruled out. We have, however, reason to believe that this is a minor problem and, hence, we ignore it in this study; see the discussion in Telhaug (1969).

compulsory schooling from 7 to 9 years and was implemented primarily over a 12 year period from 1960 to 1972 in different municipalities at different times. Thus, there is both time-series and cross-sectional variation in the number of years of compulsory schooling required of individuals during this period. We then observe the children of this generation in 2000.

Our empirical model is summarized by the following two equations:

$$ED = \beta_0 + \beta_1 ED^p + \beta_2 AGE + \beta_3 FEMALE + \beta_4 AGE^p + \beta_5 MUNICIPALITY^P \quad (1)$$

$$ED^P = \alpha_0 + \alpha_1 REFORM^p + \alpha_2 AGE + \alpha_3 FEMALE + \alpha_4 AGE^p + \alpha_5 MUNICIPALITY^P \quad (2)$$

In equations (1) and (2), ED is the number of years of education obtained by the child, AGE refers to a full set of years of age indicators, , MUNICIPALITY refers to a full set of municipality indicators, and REFORM equals 1 if the individual was affected by the education reform, and 0 otherwise. In all cases, the superscript p denotes parent, so that, for example, AGE^p refers to a full set of indicator variables for the age of the parent. We estimate the model using Two Stage Least Squares (2SLS) so that equation (2) is the first stage and $REFORM^p$ serves as an instrumental variable for ED^P.

There are a few points to note about equations (1) and (2): First, both equations contain fixed age effects and municipality effects for parents. The age effects are necessary to allow for secular changes in educational attainment over time that may be completely unrelated to the reform. For instance, there is trend in Norway as well as in other countries in this period of increased educational attainment. The municipality effects allow for the fact that variation in the timing of the reform across municipality was not randomly assigned with respect to parents' educational choice.[11] Even if the

11 In section III we argued that the available evidence suggests no systematic patterns in the timing of

reform was implemented first in areas with certain unobserved characteristics, consistent estimation is still achieved so long as (a) these characteristics are fixed over time during the 12-year period or (b) implementation of the reform is not correlated with changes in these characteristics or (c) these characteristics are not related to the schooling of the *children* of this generation.

Second, we have included age indicators for the children to allow for the fact that not all children in our sample have finished schooling by 2000. Third, we do not include any further child characteristics such as their area of residence as these are potentially endogenous to their years of schooling. For example, suppose the reform causes a woman to receive more education and this causes her to move to a city where educational costs are low and, hence, her children have high education. We consider this a causal effect of parental education on child education, even though it has arisen partly through the effect of parental education on the residence of the child.

V. Data

Based on different administrative registers and census data from Statistics Norway, a comprehensive data set has been compiled on the entire population in Norway, including information on family background, age, marital status, country of birth, educational history, neighborhood information, and employment information including wages and occupation (see Møen, Salvanes and Sørensen (2003) for a description of the data set). The initial database is linked administrative data which covers the entire population of Norwegians aged 16-74. This administrative data provides information

---

reform implementation across municipalities. However, early adopters may differ from late adopters of the reform if they value schooling more because the industrial composition in the municipality implies greater

about taxable income (excluding capital gains), educational attainment (number of years and type of education), labor market status and a set of demographic variables (age, gender). To this, we match extracts from the censuses in 1960, 1970 and 1980.[12] In order to identify the municipality the individual grew up in, we use the municipality of the individual's mother in 1960.[13] Although there is no specific family identifier, we can identify siblings as individuals with identical parents.

The measure of educational attainment is taken from a separate data source maintained by Statistics Norway; educational attainment is reported by the educational establishment directly to Statistics Norway, thereby minimizing any measurement error due to misreporting. This register provides a detailed code of the type of the highest completed education, the completion date and how many years of schooling the highest completed education corresponds to. The educational register started in 1970 so we take information for parents who completed education before then from the 1970 Census. Thus the register data are used for all but the earliest cohorts of parents that did not have any education after 1970. Census data is self reported (4 digit codes of types of education were reported) and the information is considered to be very accurate; there are no spikes or changes in the education data from the early to the later cohorts.

Our primary data source on the timing of the reform in individual municipalities was the volume by Ness (1971). To verify the dates provided by Ness (1971), we examined the data to determine whether or not there appears to be a clear break in the

---

demand for skilled workers. These types of effects are controlled for by municipality fixed effects.

[12] From the census files we have information about the age, education, income, and occupation of biological parents, along with the municipality and census tract in which the parents lived. This database covers 99% of all males, and about 97% of all females in the cohorts we are examining.

[13] Since very few children live with their father in the cases where parents are not living together, we should only have minimal misclassification by applying this rule.

fraction of students with less than 9 years of education. In the rare instance when the data did not seem to be consistent with the timing stated in Ness, we checked these individual municipalities by contacting local sources.[14] We are able to successfully calculate reform indicators for 545 out of 728 municipalities in existence in 1960. There are two main reasons for why we could not assign indicators to all municipalities. First, for some municipalities, the reform took more than one year. Second, for some municipalities we were not able to verify the information given in Ness (1971). However, we have reform information for a large majority of individuals in the relevant cohorts.

We include cohorts of parents born between 1947 and 1958 in our sample. The sample of children includes all individuals in 2000 who are aged 20 -35. Table 1 provides summary statistics for the individuals in our sample.

V. Results

Impact of Education Reform

There is a significant literature examining the impact of compulsory schooling laws on educational attainment. While in some other cases the instrument may have only a weak effect on educational attainment, the case of Norway is different in that the legislation mandated a full two year change in schooling for individuals at the bottom of the educational distribution. Table 2 demonstrates the significance of this legislation; prior to the change in the compulsory schooling laws, 12% of individuals completed less than 9 years of schooling. After the legislative change, we see that less than 3% of the

14 Between 1960 and 1970, a number of municipalities merged. In our analysis, we use the 1960 municipality as the unit of observation. In cases where the data were available at the 1970 municipality level, individual municipalities were contacted to determine the appropriate coding.

population has less than 9 years of schooling, with a new spike at 9 years.15 The rest of the distribution is relatively unchanged in the two years immediately following the reform.16

OLS Results

Because of the timing of the reform, we balance the benefits of restricting our sample to older children (who are more likely to have completed their education by 2000) against the cost in terms of losing more children whose parents were directly affected by the reform. Our primary analysis will use the sample of children who were 20-35 in 2000, but we will also try restricting our sample to 25-35 year olds as a robustness check. We will place some emphasis on whether individuals have completed 12 years of education, as, by age 20, the vast majority of individuals who will accumulate 12 years will have already done so. Note also that we are conditioning on age of child in the analysis.

The OLS results for equation (1) are presented in Table 3, Columns 1 and 3. They show, as expected, a positive relationship between the years of education of parents and their child's education, whether the latter is measured in terms of years of education or an indicator equal to one if the child completed at least twelve years of education (the

15 The presence of some individuals with less than 9 years of schooling when the reform is in place reflects both the fact that in some municipalities the reform was implemented over several years and also possibly some error in the dating of reform implementation. These factors will tend to reduce the precision of our estimates without affecting consistency.

16 Note that there is a slight decrease in the number of individuals with 10 years of education after the reform. Prior to the reform, there were also a number of individuals who attempted the traditional college preparation route but did not actually attend high school, most likely due to an inability to gain admission to the selective high school system. These students would often stay one extra year in middle school and complete their education at 10 years of schooling. In the post-reform system, this option no longer existed. In addition, a number of clerical training programs increased their duration. As a result, we suspect that many individuals who took the clerical route pre-reform would have completed 11 or 12 years of education

equivalent of a high school diploma). This is true, regardless of whether we match mothers to sons, mothers to daughters, fathers to sons, or fathers to daughters. Our estimates suggest that increasing a parent's education by a year increases the child's education by about 0.15 of a year. While the sample size varies, particularly between the father and mother regressions (due to our inability to match fathers who were not living with the family while the child was growing up), our estimates are quite similar across samples.

2SLS Results

Columns 2 and 4 then present our 2SLS results, where the instrument is the indicator for whether or not the father/mother was affected by the school reform in Norway. The 2SLS results are imprecisely estimated and are all statistically insignificant. The standard errors are sufficiently large that the 2SLS estimates are quite uninformative: one cannot rule out small or zero effects, and one also cannot generally rule out effects that are as large or larger than OLS. The main reason for the lack of precision is the weak first stage relationship between the reform and years of education of the father/mother: the t-statistics for the reform indicator in the first stage are about 5 (See Table 4 for the first stage estimates). These relatively small t-statistic result from the fact that, while the reform has a large effect on the educational distribution for individuals with 9 years and less of education, it has very minor effects at higher education levels. It is clear that to effectively use the reform as a source of exogenous variation, one needs to focus on the very bottom tail of the education distribution, where the reform has bite.

In Tables 5 and 6, the sample is restricted to mothers/fathers who have 9 or fewer

---

post-reform.

years of education. The additional assumptions we make in doing this are 1) that individuals who get 9 years of education after the reform would have received 9 years or less of education before the reform, and 2) that individuals who got 9 years or less or education before the reform would have received 9 years of education if the reform had been in effect.17 The fact that the proportion of individuals with 9 years or less of education stays constant when we compare two years before to two years after the reform (see table 2), suggests that our assumptions are not unreasonable. In return for making these additional assumptions we estimate a much stronger first stage (see table 6) and more precisely estimated second stage coefficients.

The OLS estimates for the sample of parents with 9 or fewer years of education are in Columns 1 and 3 of table 5. These are reasonably similar in magnitude to the OLS estimates for the full sample but tend to be a little higher.18 The 2SLS estimates (Columns 2 and 4) are, as expected, much more precisely estimated than in the full sample. They are also universally smaller than the OLS estimates. This is particularly true for fathers: the estimates are all close to zero and statistically insignificant. For mothers, the evidence suggests a positive effect of maternal education on the education of sons but no such relationship for daughters. Even the statistically significant mother/son coefficients are smaller than the OLS coefficients. Taken as a whole, the results indicate that the positive correlation between parent's and child's education largely represents positive relationships between other factors that are correlated with education. These

17 This second assumption rules out spillover effects of the reform of the sort that some signaling models imply.

18 Note that these OLS estimates are more similar to the 2SLS estimates than would be the case from a conventional cross-sectional OLS regression. This is because individuals with 9 years of education post-reform may have chosen fewer years of education in the absence of the reform. Hence, the positive correlation between education and unobserved ability that would tend to bias the OLS coefficients upwards is reduced by the presence of the reform. Indeed when we carry out OLS regression using only pre-reform

could be ability, family background, income or other factors. The true causal effect of parental education on child education appears to be close to zero.

Our finding that the IV estimates are smaller than the OLS estimates is not in keeping with much of the returns to education literature. Typically, IV estimates are found to be larger than OLS estimates. We suspect a few reasons for this divergence. First, our education data is of very high quality and probably has little or no measurement error. This is in contrast to the self-reported education data used in most studies. Thus, unlike in other studies, our OLS estimates are not subject to downward biases due to measurement error. Second, our use of an education reform and our ability to control for both age and municipality effects leads to greater confidence that the instrument is not correlated with unobserved ability and hence our IV estimates are not upward biased. Third, high IV estimates in the endogenous education literature are often rationalized by heterogeneous returns to education with particularly high returns for the group of people whose behavior is impacted by whatever instrument is used. Because credit constraints are unlikely to have been a major determinant of educational choice in the lower tail of the Norwegian distribution at this time, it is plausible that the returns to education for individuals impacted by the reform are no higher than the average. This contention is supported by the fact that the OLS coefficients for the full sample are similar to the OLS coefficients for the sample where parents have less than 10 years of schooling.

VI. Robustness/Specification Checks

Sample Selection

Given the population of data available from Statistics Norway, we were able to

observations, we find larger OLS coefficient values.

avoid many potential problems of sample selection. One selection criterion we do implement is to use children who are aged between 20-35. By doing this, we may be including children who have not yet finished their educational experience at the time of the survey. Given that our instrument is effective for the lower end of the educational distribution (and less educated parents tend to have less educated children), it is unlikely that we are biasing our results much by including the younger children. However, as a sensitivity check, we estimate the relationship between parents' and children's education on a subsample of 25-35 year olds. These results are presented in Tables 7-10. Our OLS estimates of the effect of years of parental education are slightly higher than before (approximately .21), but our IV estimates are essentially the same. In our preferred specification, limited to the subsample of parents with less than 10 years of completed education, we see the same positive and significant relationship for mothers and sons but no relationship for mothers and daughters or fathers and sons or daughters.

Specification Check: Highly Educated

The Norwegian education reform increased compulsory schooling from 7 to 9 years. As a result, we should see no effect of the reform on the educational attainment of more highly educated individuals. To verify this, we estimated our first and second stages on a subsample of individuals whose parents had completed at least 12 years of education. These results are presented in Tables 11 and 12. As expected, the first stage has no predictive power and the IV results are meaningless.

Specification Check: Selection Into Parenthood

Another potentially serious concern is the selection of individuals into parenthood. For example, suppose women with low human capital are more likely to become mothers, and this also varies by ability. That is, conditional on human capital, high ability women are less likely to have children. If the reform raises the human capital of a subgroup of women, then the high ability women in that subgroup may now choose not to have children. This would imply that the average ability level of mothers subject to the reform is lower than the average ability of mothers not subject to the reform. Hence, our IV results would be biased towards zero. One check on this possibility is to examine whether the reform affects the probability that an individual becomes a parent. If the reform has no impact on this probability, then it is unlikely to affect the sorting into parenthood by ability level.

Table 13 presents the results when we estimate a linear probability model on the sample of all individuals born between 1947-1958 with the dependent variable equal to one if the individual has one or more children in our sample of 20-35 year olds. Our variable of interest is the reform indicator. As always, we include municipality and cohort indicators in the specification. The results suggest that there is no relationship between the reform and the likelihood an individual has a child in our sample; the estimated coefficients are both economically small and statistically insignificant.

Sibling Fixed Effects

The use of sibling fixed effects is common in the literature. While we are not convinced by the argument that education is exogenous conditional on these fixed effects, we consider it a useful exercise to see how inclusion of these controls affects the OLS

results. If parent's education is positively related to unobserved ability, we would expect that inclusion of sibling fixed effects would reduce the OLS coefficients.

Table 14 presents the results when we estimate the relationship between parents' and children's education on same-sex sibling pairs of parents (Note that both siblings must have children in our sample to be included.) Despite this seemingly restrictive sample selection, our sample sizes were still quite large, with at least 40,000 of each parent-child sex pairing. The OLS estimates are quite similar to those estimated on our full sample, and the fixed effects estimates are uniformly lower than the OLS estimates. Given that measurement error is not a major problem with our education data, we interpret these findings as suggesting that the OLS estimates are biased upwards because more able children or children with better family background are more likely to obtain more schooling. We don't put much faith in the siblings fixed effects estimates themselves because siblings with different levels of education are likely to have different ability levels. However, we see the estimates as providing some more evidence that the true causal effect of parental education is much lower than suggested by OLS estimates.

VII. Interpretation of the Results

In the previous section, we found no evidence of a causal effect of father's education on child education but some evidence that mother's education affects the education of sons. Although theory provides very little guidance on this, we consider it plausible that mothers' education may matter more than fathers' given that children tend to spend much more time with their mother. We have no explanation as to why mother's

education may be more important to their sons than their daughters.19 In this section, we explore the mechanisms through which mothers' education may affect child education. There are many possible mechanisms and we have limited information with which to disentangle them so the tone here is very speculative.

The most direct mechanism is that extra schooling increases human capital of the mother and directly increases the optimal human capital choice of children. This could arise because the return to human capital is higher for individuals who have more educated parents or because the costs of human capital accumulation are lower (maybe because their parents can help them more with their schoolwork). While this direct mechanism is plausible, there are many alternative routes by which mothers' human capital can transmit into greater child education:

One possibility is that more educated women marry men who have more education, higher income, and greater ability than the men married by less educated women. If so, increasing a woman's education level may lead her children to have more education because of genetic transmission of traits from the chosen husband. Even in the absence of genetic transmission, the husband's characteristics may impact the child's choice of schooling level. While we know that in general more educated women marry men with higher education and income levels, we now check whether this factor is driving our 2SLS results. To do so, we regress father's education and earnings on mother's education using the reform as instrument for mother's education. These results are presented in Table 15. We find the 2SLS estimates of the effects of mother's education on father's education and log earnings to be small and statistically insignificant for both the full sample and the sample of mothers with education of less than 10 years.

19 Interestingly, Chevalier (2003) also finds this result using British data.

In contrast, the OLS estimates are significantly positive in each case.20

The above evidence suggests that greater education induced by the reform did not have any major effect on the type of father chosen by the mother. This does not necessarily imply that it is mother's human capital per se that influences the child. Women with higher education have higher earnings and it may be the extra earnings that affect the child. Unfortunately, given we only have one instrument, we cannot distinguish whether it is the education itself or the extra earnings induced by the extra education that matters.

Yet another mechanism that could be at work is quality/quantity tradeoff. Women with more education may choose to have fewer children and to invest more in each one of them. In future work, we hope to further examine the mechanisms through which this relationship works.

VIII. Conclusions

Despite strong OLS relationships, we find little causal relationship between parent education and child education. The one exception is among mothers and sons; when mothers increase their educational attainment, their sons will get more education as well. Overall, however, these results suggest that intergenerational spillovers may not be a compelling argument for subsidizing education. However, it is important to remember that we are studying an education reform that increased education at the bottom tail of the distribution. It is plausible that a policy change that increased enrollment in higher education would have been transmitted more successfully across generations. Much more

20 We have also tried adding fathers education as an extra control variables in the 2SLS estimation of child education on mothers education and found that the coefficient on mother's education falls very slightly.

work needs to be done on this important topic.

References

Aakvik, Arild, Kjell G. Salvanes, and Kjell Vaage. 2003. "Measuring the Heterogeneity in the Returns to Education in Norway Using Educational Reforms." CEPR DP 3995.

Antonovics, Kate L. and Arthur S. Goldberger. 2003. "Do Educated Women Make Bad Mothers? Twin Studies and the Intergenerational Transmission of Human Capital." Mimeo.

Bjørklund, Anders and Markus Jantti. 1997. "Intergenerational Income Mobility in Sweden Compared to the United States." *American Economic Review*, 87(5), pp 1009-1018.

Björklund, Anders, Tor Eriksson, Markus Jännti, Oddbjørn Raaum and Erik Österbacka. 2002. "Brother correlations in earnings in Denmark, Finland, Norway and Sweden compared to the United States." *Journal of Population Economics*

Bratberg, Espen, Øivind Anti Nilsen, and Kjell Vaage. 2002. "Assessing Changes in Intergenerational Earnings Mobility." Centre for Economic Studies in Social Insurance Working Paper No. 54, University of Bergen, December.

Behrman, Jere R. and Mark R. Rosenzweig. 2002. "Does Increasing Women's Schooling Raise the Schooling of the Next Generation?" *American Economic Review*, 91(1), pp 323-334.

Bound, John and Gary Solon. 1999. "Double Trouble: On the Value of Twins-Based Estimation of the Returns to Schooling." *Economics of Education Review*, 18, pp 169-182.

Currie, Janet and Enrico Morietti. 2003. "Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings." *Quarterly Journal of Economics, forthcoming.*

Chevalier, Arnaud. 2003. "Parental Education and Child's Education: A Natural Experiment." Mimeo, University College Dublin.

Dearden, L, S. Machin and H. Reed. 1997. "Intergenerational Mobility in Britain." *Economic Journal*, 107, 47-66.

Griliches, Zvi. 1979. "Sibling Models and Data in Economics: Beginnings of a Survey." *Journal of Political Economy*, 87, pp 37-64.

Harmon, C. and Ian Walker. 1995. "Estimates of the Economic Return to Schooling for the United Kingdom." *American Economic Review*, 85, 1278-86.

Lie, Suzanne S. (1973) The Norwegian Comprehensive School Reform. Strategies for Implementation and Complying with Regulated Social Change. A Diffusion Study. Part 1 and II. Washington, D.C., The American University.

Lie, Suzanne S. (1974) ”Regulated Social Change: a Diffusion Study of the Norwegian Comprehensive School Reform”, *Acta Sociologica*, 16(4), 332-350.

Lindbekk, Tore (1992) ``School Reforms in Norway and Sweden and the Redistribution of Educational Attainment." *Journal of Educational Research*, 37(2), 129-49.

Meghir, Costas and Marten Palme. 2003. “Ability, parenthal background and education policy: empirical evidence from a social experiment”, mimeo, Stockholm School of Economics.

Mulligan, C. 1999. “Galton versus the human capital approach to inheritance.” *Journal of Political Economy*, 107, 184-224.

Møen, Jarle, Kjell G. Salvanes and Erik Ø. Sørensen. 2003. “Documentation of the Linked Empoyer-Employee Data Base at the Norwegian School of Economics.” Mimeo, The Norwegian School of Economics and Business Administration.

Oreopoulos, P. 2003. “Do dropouts drop out too soon?” Mimeo, University of Toronto,.

Plug, Erik. 2002. "How do Parents Raise the Educational Attainment of Future Generations?", IZA Discussion Paper No. 652.

Raaum, O., Salvanes, K.G. and Sørensen, E. Ø. (2001). ”The Neighbourhood is not what it used to be. Discussion paper 36/2001, The Norwegian School of Economics and Business Administration.

Raaum, Oddbjørn, Kjell G. Salvanes, Erik Ø. Sørensen (2003) “The Impact of a Primary School Reform on Social stratification: A Norwegian study of Neighbour and School Mate Correlations." Mimeo, The Norwegian School of Economics and Business Administration.

Table 1: Summary Statistics

	Observations	Mean	Std. Dev.
Children			
Age	351,086	24.80	3.50
Education	351,086	11.94	1.61
Earnings	351,086	159,594	124,842
Sex (Female / Male)	351,086	0.49	0.50
Mothers			
Age	286,137	48.34	3.12
Education	286,137	10.88	2.37
Earnings	282,922	182,017	120,774
Fathers			
Age	228,060	49.29	2.86
Education	228,060	11.43	2.66
Earnings	222,405	331,301	506,507

Table 2: Distribution of Education Two Years Before and After the Reform

Years of Education	Before	After
7	3.5 %	1.2 %
8	8.9 %	1.6 %
9	3.4 %	12.9 %
10	29.5 %	26.6 %
11	8.5 %	8.9 %
12	17.2 %	19.1 %
13	6.7 %	6.6 %
14	5.4 %	5.8 %
15	2.7 %	3.4 %
16+	14.2 %	14.1 %
N	89,320	92,227

Table 3: Relationship between Parents' and Children's Education
Full Sample

	Dependent Variable			
	Years of Education		Education > 12 Years	
	OLS	IV	OLS	IV
Mother – All	0.1578 *	0.0414	0.0388 *	-0.0018
N = 286,137	(0.0022)	(0.0955)	(0.0004)	(0.0283)
Mother – Son	0.1459 *	0.0202	0.0391 *	0.0052
N = 147,005	(0.0026)	(0.1260)	(0.0005)	(0.0406)
Mother – Daughter	0.1703 *	0.0550	0.0385 *	-0.0087
N = 139,132	(0.0025)	(0.1259)	(0.0005)	(0.0356)
Father – All	0.1387 *	0.0343	0.0368 *	0.0156
N = 228,060	(0.0016)	(0.0745)	(0.0004)	(0.0236)
Father – Son	0.1369 *	0.1034	0.0388 *	0.0561
N = 117,372	(0.0019)	(0.0889)	(0.0005)	(0.0316)
Father – Daughter	0.1408 *	-0.0420	0.0347 *	-0.0337
N = 110,688	(0.0020)	(0.1132)	(0.0005)	(0.0353)

Sample includes children aged 20-35. Robust standard errors in parentheses.
All specifications include dummies for parent's age, parent's municipality, and child's age.
*significant at 5%

Table 4: First Stage Results
Full Sample

	Mother's Education	Father's Education
All	0.1133 *	0.1595 *
	(0.0208)	(0.0291)
Son	0.1058 *	0.1689 *
	(0.0249)	(0.0343)
Daughter	0.1219 *	0.1527 *
	(0.0248)	(0.0346)

Robust standard errors in parentheses. First stage also includes dummies for parent's age, parent's municipality, and child's age. *significant at 5%

Table 5: Relationship between Parents' and Children's Education
Parents with <10 Years Education

	Dependent Variable			
	Years of Education		Education > 12 Years	
	OLS	IV	OLS	IV
Mother – All	0.1844 *	0.0786 *	0.0509 *	0.0197
N = 63,269	(0.0130)	(0.0316)	(0.0040)	(0.0103)
Mother – Son	0.1743 *	0.1051 *	0.0484 *	0.0206
N = 32,247	(0.0158)	(0.0388)	(0.0052)	(0.0136)
Mother – Daughter	0.1935 *	0.0480	0.0521 *	0.0166
N = 31,022	(0.0182)	(0.0444)	(0.0055)	(0.0136)
Father – All	0.1642 *	0.0468	0.0462 *	0.0126
N = 43,407	(0.0132)	(0.0344)	(0.0043)	(0.0118)
Father – Son	0.1403 *	0.0366	0.0406 *	0.0150
N = 22,059	(0.0166)	(0.0402)	(0.0060)	(0.0154)
Father – Daughter	0.1844 *	0.0516	0.0515 *	0.0099
N = 21,348	(0.0201)	(0.0509)	(0.0064)	(0.0162)

Robust standard errors in parentheses
All specifications include dummies for parent's age, parent's municipality, and child's age.
*significant at 5%

Table 6: First Stage Results
Parents with <10 Years of Education

	Mother's Education	Father's Education
All	0.7329 *	0.7777 *
	(0.0155)	(0.0204)
Son	0.7142 *	0.7881 *
	(0.0173)	(0.0229)
Daughter	0.7504 *	0.7702 *
	(0.0172)	(0.0225)

Robust standard errors in parentheses. First stage also includes dummies for parent's age, parent's municipality, and child's age. *significant at 5%

Table 7: Relationship between Parents' and Children's Education
Full Sample
25-35 Year Old Children

	Dependent Variable			
	Years of Education		Education > 12 Years	
	OLS	IV	OLS	IV
Mother – All	0.2373 *	0.0760	0.0413 *	0.0459
N = 143,579	(0.0031)	(0.1393)	(0.0006)	(0.0344)
Mother – Son	0.2123 *	0.1988	0.0380 *	0.0949
N = 73,663	(0.0041)	(0.1850)	(0.0008)	(0.0531)
Mother – Daughter	0.2636 *	-0.0294	0.0449 *	0.0043
N = 69,916	(0.0038)	(0.1865)	(0.0007)	(0.0423)
Father – All	0.2171 *	0.0300	0.0375 *	-0.0167
N = 96,275	(0.0027)	(0.1322)	(0.0006)	(0.0333)
Father – Son	0.2090 *	0.0288	0.0365 *	0.0105
N = 49,492	(0.0036)	(0.1713)	(0.0008)	(0.0462)
Father – Daughter	0.2263 *	0.0225	0.0388 *	-0.0490
N = 46,783	(0.0037)	(0.1859)	(0.0008)	(0.0466)

Robust standard errors in parentheses
All specifications include dummies for parent's age, parent's municipality, and child's age.
*significant at 5%

Table 8: First Stage Results
Full Sample of Parents
25-35 Year Old Children

	Mother's Education	Father's Education
All	0.1420 *	0.1923 *
	(0.0293)	(0.0423)
Son	0.1271 *	0.1963 *
	(0.0346)	(0.0508)
Daughter	0.1606 *	0.1969 *
	(0.0359)	(0.0501)

Robust standard errors in parentheses. First stage also includes dummies for parent's age, parent's municipality, and child's age. *significant at 5%

Table 9: Relationship between Parents' and Children's Education
Parents with <10 Years Education
25-35 Year Old Children

	Dependent Variable			
	Years of Education		Education > 12 Years	
	OLS	IV	OLS	IV
Mother – All	0.2111 *	0.1216 *	0.0546 *	0.0319 *
N = 40,098	(0.0169)	(0.0432)	(0.0048)	(0.0130)
Mother – Son	0.1970 *	0.1764 *	0.0512 *	0.0480 *
N = 20,135	(0.0213)	(0.0544)	(0.0065)	(0.0177)
Mother – Daughter	0.2246 *	0.0662	0.0569 *	0.0162
N = 19,470	(0.0235)	(0.0631)	(0.0064)	(0.0175)
Father – All	0.2000 *	0.0408	0.0488 *	0.0133
N = 22,148	(0.0215)	(0.0618)	(0.0061)	(0.0176)
Father – Son	0.1513 *	0.0078	0.0381 *	0.0129
N = 11,235	(0.0274)	(0.0714)	(0.0086)	(0.0237)
Father – Daughter	0.2438 *	0.0806	0.0581 *	0.0067
N = 10,913	(0.0329)	(0.0936)	(0.0089)	(0.0249)

Robust standard errors in parentheses
All specifications include dummies for parent's age, parent's municipality, and child's age.
*significant at 5%

Table 10: First Stage Results
Parents with <10 Years of Education
25-35 Year old Children

	Mother's Education	Father's Education
All	0.7494 *	0.7952 *
	(0.0166)	(0.0240)
Son	0.7419 *	0.8139 *
	(0.0194)	(0.0287)
Daughter	0.7552 *	0.7792 *
	(0.0192)	(0.0274)

Robust standard errors in parentheses. First stage also includes dummies for parent's age, parent's municipality, and child's age. *significant at 5%

Table11: Relationship between Parents' and Children's Education
Parents with at Least 12 Years of Education

	Dependent Variable			
	Years of Education		Education > 12 Years	
	OLS	IV	OLS	IV
Mother – All	0.1080 *	-0.3900	0.0204 *	-0.0161
N = 89,193	(0.0031)	(0.3537)	(0.0008)	(0.0728)
Mother – Son	0.0994 *	-0.5182	0.0213 *	-0.1037
N = 46,056	(0.0041)	(0.6943)	(0.0012)	(0.1808)
Mother – Daughter	0.1180 *	-0.3235	0.0193 *	0.0309
N = 43,137	(0.0046)	(0.3575)	(0.0010)	(0.0726)
Father – All	0.1255 *	1.1959	0.0280 *	0.6184
N = 106,989	(0.0026)	(3.5369)	(0.0006)	(1.7587)
Father – Son	0.1262 *	2.7932	0.0307 *	42.5819
N = 55,359	(0.0033)	(13.6391)	(0.0009)	(12.5947)
Father – Daughter	0.0795 *	-0.2261	0.0130 *	-0.0913
N = 25,446	(0.0047)	(0.9185)	(0.0011)	(0.2593)

Robust standard errors in parentheses
All specifications include dummies for parent's age, parent's municipality, and child's age.
*significant at 5%

Table 12: First Stage Results
Parents with at Least 12 Years of Education

	Mother's Education	Father's Education
All	-0.0648 *	0.0102
	(0.0293)	(0.0290)
Son	-0.0445	0.0073
	(0.0358)	(0.0360)
Daughter	-0.0860 *	-0.0395
	(0.0355)	(0.0504)

Robust standard errors in parentheses. First stage also includes dummies for parent's age, parent's municipality, and child's age. *significant at 5%

Table 13:
Probability of Having a Child in the Sample

	Full Sample	Education < 10 years
Mothers	-0.0038	0.0009
	(0.0040)	(0.0066)
	N = 260,674	N = 42,026
Fathers	0.0077	-0.0018
	(0.0040)	(0.0078)
	N = 292,749	N = 45,591

Robust standard errors in parentheses
All specifications include dummies for parent's age and parent's municipality.
*significant at 5%

Table14: Relationship between Parents' and Children's Education
Sibling Fixed Effects

	Dependent Variable			
	Years of Education		Education > 12 Years	
	OLS	FE	OLS	FE
Mother – All	0.1419 *	0.0725 *	0.0364 *	0.0166 *
N = 90,902	(0.0022)	(0.0038)	(0.0006)	(0.0011)
Mother – Son	0.1330 *	0.0681 *	0.0374 *	0.0168 *
N = 46,889	(0.0029)	(0.0057)	(0.0008)	(0.0018)
Mother – Daughter	0.1515 *	0.0798 *	0.0355 *	0.0149 *
N = 44,013	(0.0032)	(0.0064)	(0.0008)	(0.0017)
Father – All	0.1345 *	0.0731 *	0.0373 *	0.0185 *
N = 82,894	(0.0019)	(0.0036)	(0.0005)	(0.0011)
Father – Son	0.1361 *	0.0823 *	0.0399 *	0.0218 *
N = 42,736	(0.0025)	(0.0054)	(0.0008)	(0.0018)
Father – Daughter	0.1329 *	0.0579 *	0.0345 *	0.0136 *
N = 40,158	(0.0027)	(0.0063)	(0.0007)	(0.0018)

Regressions include same-sex sibling pairs. Robust standard errors in parentheses.
All specifications include dummies for parent's age and child's age.
*significant at 5%

Table 15: Relationship between Mother's Education and Father's Earnings/ Education

Mother's Education	Dependent Variable			
	Father's Education		Log Father's Earnings	
	OLS	IV	OLS	IV
Full Sample	0.5042 *	0.1927	0.0408 *	0.0323
	(0.0061)	(0.2814)	(0.0009)	(0.0644)
	N = 102,898	N = 102,898	N = 93,452	N = 93,452
Education < 10 Years	0.1840 *	0.0465	0.0355 *	0.0028
	(0.0298)	(0.0664)	(0.0096)	(0.0220)
	N = 19,107	N = 19,107	N = 16,549	N = 16,549

Robust standard errors in parenthesis
All specifications include dummies for mother's age and mother's municipality.
*significant at 5%

Figure 1
The Number of Municipalities Implementing the Education Reform, by Year

Figure 2
Reform implementation in Poor vs Rich Municipalities
Based on Average Family Income.

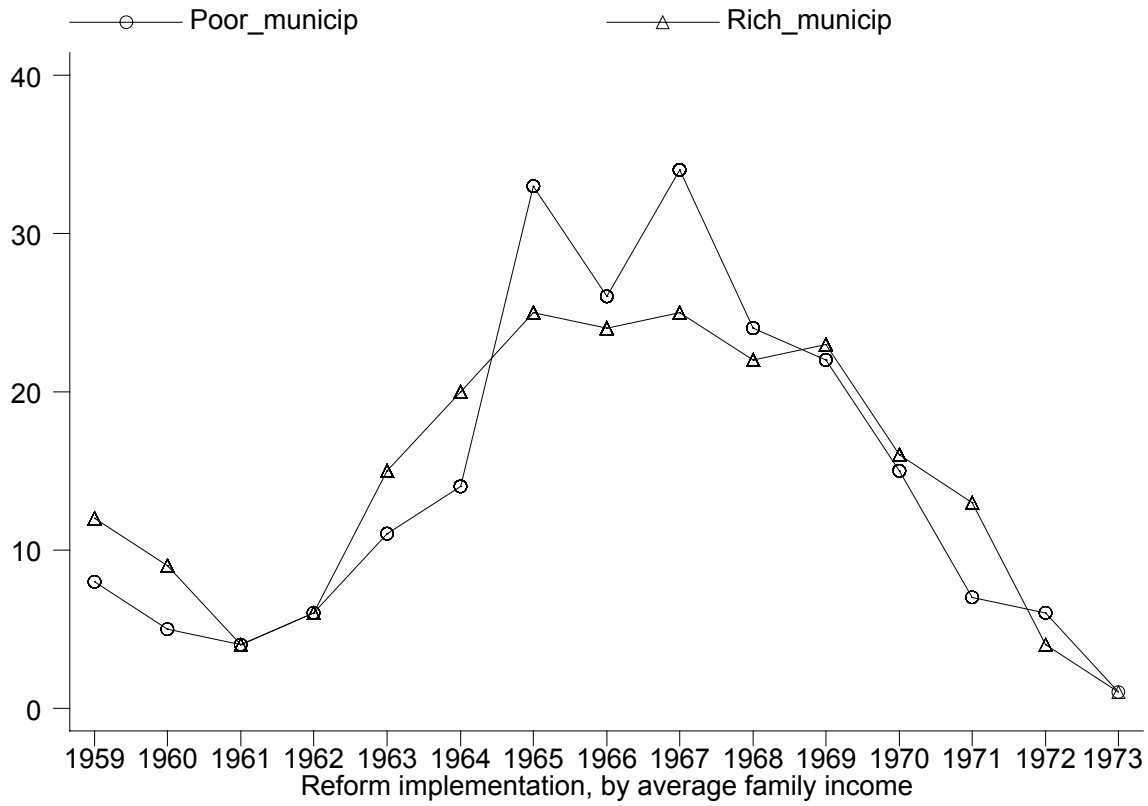


Figure 3
Reform Implementation in High vs. Low Education Municipalities
Based on Average Years of Education in the Municipality

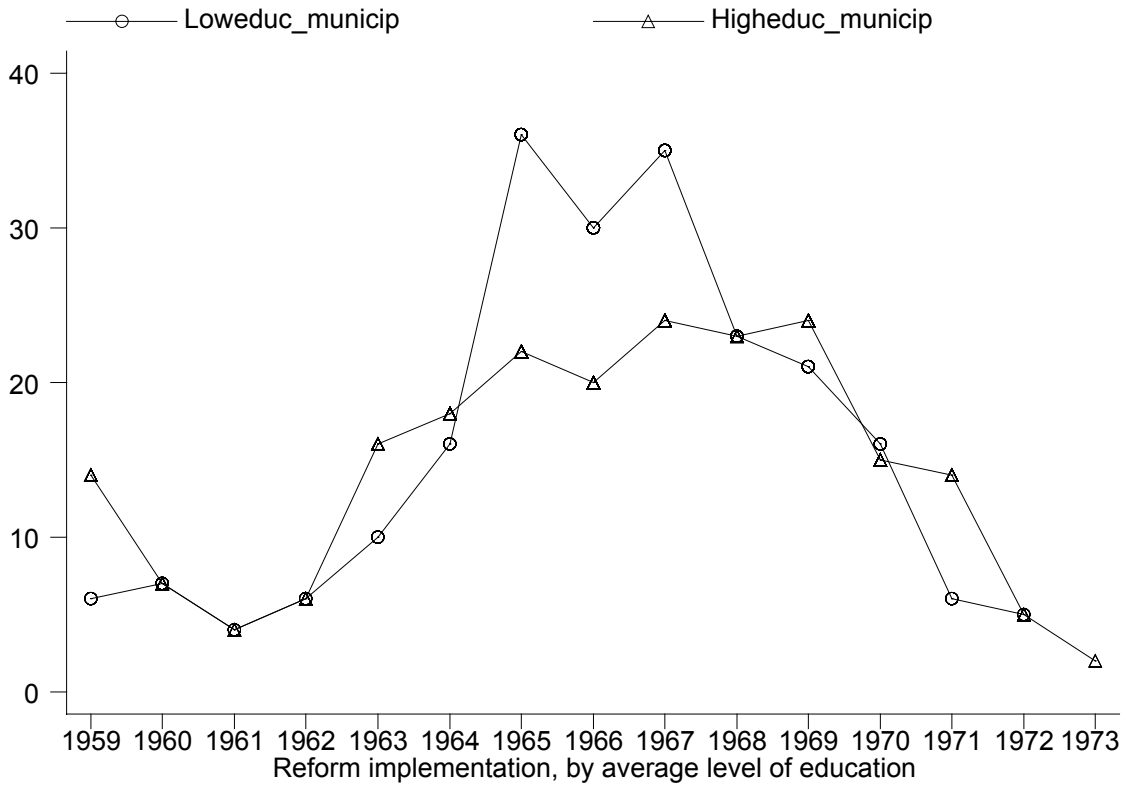
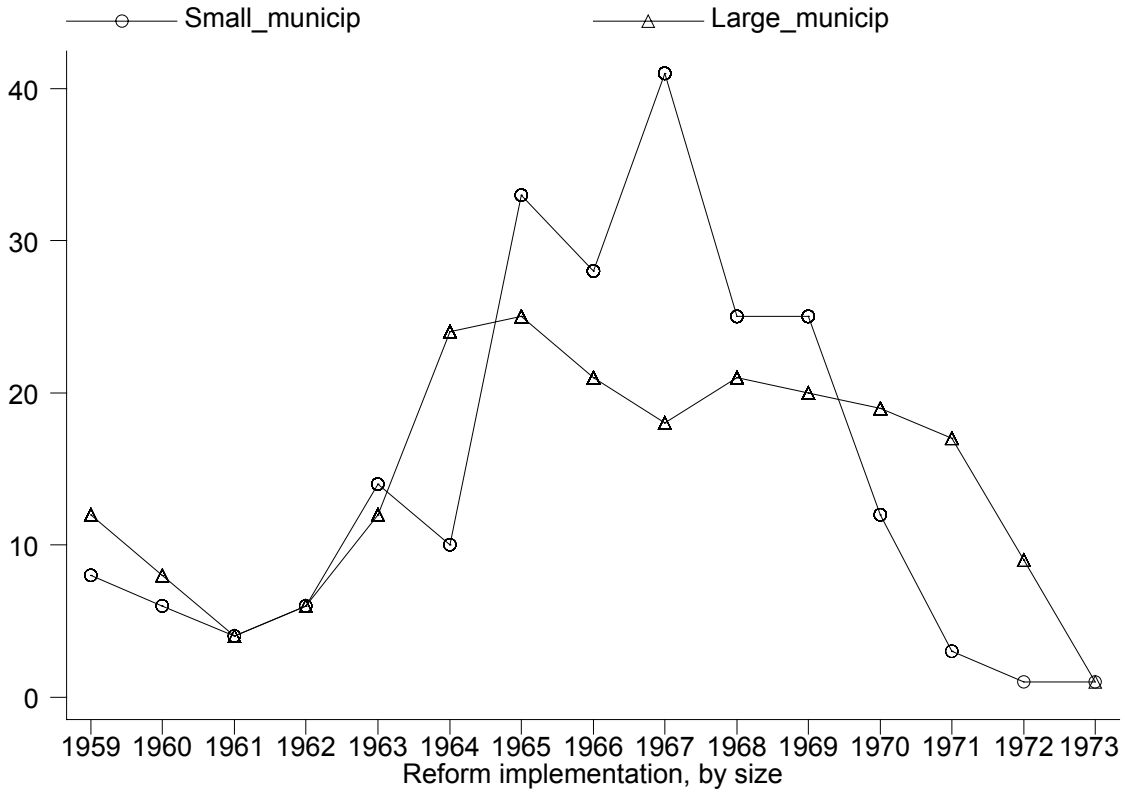


Figure 4
Reform Implementation in Small vs. Large Municipalities



Appendix Table 1:
Timing of the Implementation of the Reform

Dependent Variable: Year of Reform

	Coefficient	Standard error
County2	-2.0084	0.6361
County3	4.6204	5.1743
County4	-0.5769	0.6302
County5	-0.8327	0.6264
County6	-0.7722	0.6161
County7	-1.2283	0.6241
County8	-1.8233	0.6367
County9	-1.2159	0.6284
County10	-2.2932	0.6748
County11	-0.8081	0.5696
County12	-1.6951	0.5417
County13	-0.5687	0.6689
County14	1.0369	0.5459
County15	-1.5363	0.5518
County16	0.0186	0.5608
County17	-1.2770	0.5311
County18	-0.2418	0.5973
County19	-2.0084	0.6361
Father college	2.4052	3.6445
Mother college	10.732	8.2210
Father income	-0.0059	0.0039
Mother income	-0.0080	0.0079
Father age	0.0499	0.1510
Mother age	-0.0936	0.1872
Size municipality/100	-0.0277	0.0305
Constant term	1970.9	6.6907